



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

*Two Letters from Professor Porter in regard to the Bod-
‘Astart stones in Beirut.*—By Professor CHARLES C.
TORREY.

THE former of the two letters was written from Beirut in February, 1904, by Professor Harvey Porter, of the Syrian Protestant College. It was called out by the chapter entitled, "A New Inscription from the Temple of Ešmūn," in my article *Semitic Epigraphical Notes*, published in vol. xxiv. of this Journal, pages 218–226, and aimed to show, by an appeal to the original stones themselves, that the restoration of the inscription there proposed is untenable. The letter was addressed to the editors of the Journal. Professor Porter writes: "There is a difficulty in regard to the first legible letter in the first line, which is clearly a י, and Dr. Torrey would solve it by making it a מ, the final letter of צרנם. But the downward stroke to the right of this י cannot belong to a י, as he conjectures, but to a ת, the horizontal stroke of which is plainly visible on the stone and still contains traces of the red coloring matter with which all the letters were originally stained. Hence Dr. Schröder's conjecture that this letter is ת is perfectly correct, so that Dr. Torrey's restoration of the first line must be in error. Moreover, at the right of this letter where Dr. Torrey places two marks which he thinks might be portions of a ך there is absolutely nothing like the trace of a letter, the damage to the stone having destroyed all such traces.

Also to the right of this space, about the distance of another letter, there is a downward, nearly perpendicular, stroke, perfectly clear, which might belong to a ת, or מ, or כ, but hardly to a ז, as it inclines the wrong way. From this point on to the right the stone has been so badly damaged that nothing can be made out, but Dr. Schröder's conjecture that the word preceding the י is בעשהרת seems more in accord with the traces on the stone than Dr. Torrey's restoration.

In regard to the two fragments upon which occur the words צרנם and ית, they undoubtedly belong together one above the other, as will be seen by the rough drawing I enclose. The

broken letters of the upper fragment are exactly matched by the traces on the upper portion of the lower fragment. It should be noted, however, that the space at the right of the first letter on the upper one is at least twice the space between letters, and the stone here has not been defaced and could never have contained a letter. Hence, if this fragment precedes the word **בן** in the second line on the main stone, it must have begun the line, and thus Dr. Torrey's conjecture that the line began with **מלך ם** cannot be correct. Likewise, on the lower fragment, there is no trace of an **א** at the right of the ' as there might be I should think from the space; so it seems to me that neither the condition of the main stone nor the fragments bear out Dr. Torrey's emendations."

The second letter, which is dated June 6, 1904, contains Professor Porter's own restoration of the mutilated inscription, employing the two small fragments, which he also now believes to have originally joined the larger fragments at the right hand, supplying partially the beginning of the second and third lines. In regard to this he writes: "Supposing that the two small fragments belong to the larger right-hand fragment (which I think most probable), and that the one which has **צדנם** on it begins a line (as it must, I think, since there is no trace of any letter before it although the blank space is more than sufficient for one), then the last-named fragment would naturally fall at the beginning of the second line of the inscription. If this word were followed directly by the **בן בן** on the larger fragment, we should have space in the first line for **מלך ברעשתרת** before **ו** which is the first letter there distinctly legible. The **ת** of the other small fragment would then fall before **הבת** in the third line, and we should have:

מלך · ברעשתרת[ו] · בן · צדק · יתנמלך · מלך
 צדנם · בן · בן · מלך · אשמנעור · מלך · צדנם
 [א] · ת · הבת · ז · בן · לאלי · לאשמן · שר · קדש

This supposes the name of the father of Bodastoreth to read Yatan Melek, instead of Ṣadiḳ-Yatan as I at first supposed, and makes Ṣadiḳ an attribute to *ben*, i. e., "genuine son." Is it possible for the whole combination Ṣadiḳ-Yatan-Melek to be a name? I know of no such combination in Phoenician names,

but judging from the Hebrew I should think it might be. This reading gets rid of the grandiloquent title "king of kings," so little appropriate to a king of Sidon at that period. The difficulty of the י in the first line, which is unmistakable, still remains, and I cannot solve it, except by supposing it to be a mistake of the engraver and that it is to be ignored in reading. But that is hardly a satisfactory solution."

I need not say that it is a source of especial gratification to me that the scholar who is best acquainted with the stones themselves has adopted my suggestion that the two small fragments in the Beirut museum belong to an inscription of Bod-ʿAstart, and, what is more, that they and the two large pieces originally belonged to one and the same block. In fact, this now seems to be established beyond all doubt. Professor Porter shows that the two small pieces, when put one above the other, match exactly, the lower part of the shafts of the letters ם and ן in ןרנם on the one fragment being plainly visible on the other, and the distance between the two lines thus formed corresponding to the space between the lines on the larger stone.

Professor Porter's information in regard to the condition of the large right-hand fragment is especially welcome. In one point he rectifies my reading of the photograph which was my sole (and of course often precarious) authority. The marks which I saw at the distance of two letters to the right of the י in the first line, and supposed to belong to the head of a ך, prove to have been nothing more than recent scratches on the stone. However, as all the other restorations of the line, including Professor Porter's own, supply the same letter at this point (calling it ך), the correction does not necessarily affect my reading.

More important is the statement in regard to the letter just at the right of the י, which Professor Porter and Consul Schröder both believe to have been a ף. A part of the horizontal cross-bar, it appears, is still distinctly visible, with traces of the red paint with which all the letters were originally colored. Regarding this horizontal mark, I hope to have later a still more definite statement; for the present, however, I must confess myself still unconvinced. Dr. Schröder had previously written to me in

regard to this letter, saying that it "was probably a **Ⲛ**, the traces of the red colour of the cross line of this letter (*on the left of the upper part of the shaft*) being yet visible." The italics are my own. Unless the word "left" in this description is a mistake for "right," the cross line in question deserves to be viewed with strong suspicion. Is it customary in this inscription—or in its fellows—for the letter **Ⲛ** to have its cross-bar continued to the left of the main shaft? And again, if the horizontal mark is really thus situated, why may it not belong to a **ⲛ**, the cross stroke of which would occupy exactly this position? Another consideration makes me hesitate still more. While one of the inscribed blocks from the temple of Ešmūn was in my possession, in Sidon, I found out to my dismay that the red paint which had filled the letters had also soaked into the adjacent surface of the stone. More or less recent abrasions often showed red, and I very soon learned by experience that I could not safely determine the original course of a doubtful letter by scratching for red paint, which could be found in places where it was quite certain that no letter had ever been. And finally—to repeat a fact to which I called attention in my former article—the lower part of the shaft of the letter in question is distinctly *curved to the left*, a peculiarity which belongs regularly only to the letters **ⲛ** and **ⲛ**. The resemblance which this particular shaft bears to that of the **ⲛ** which almost immediately follows it in the same line is most striking. So far as my two photographs can show, they are absolutely identical in form, and distinctly unlike the shaft of the **Ⲛ** in the two places where it can be seen in this inscription. Hence it still seems to me—though I may eventually be proved wrong in this—that the reading of **Ⲛ** at this point is less probable, on the evidence of the traces actually remaining, than that of **ⲛ**.

Again, it appears that at the space of about two letters to the right of this character there is the distinct remnant of the shaft of a letter which had the slant of **Ⲛ** or **ⲛ**. Professor Porter supposes this to be a part of the first **Ⲛ** in the word **ⲉⲥⲏⲧⲏⲧⲏ**. I am unable to locate this shaft with any certainty on either of my photographs; this, however, is a matter of small concern, for Professor Porter pronounces the space between the two shafts at least sufficient for the letter **ⲛ** and the rest (the right-hand portion) of the second **Ⲛ**; and with this type of the

Phoenician alphabet it is pretty certain that the space thus occupied could equally well contain the letters צד. I therefore believe the stroke in question to be the shaft of the letter ך in the word מלך.

The restoration of the inscription proposed by Professor Porter does not seem to me to be in all respects a plausible one. In the first place, it does not give to Bod-‘Astart the title “King of the Sidonians.” The מלך צדנם at the end of the first line and the beginning of the second can only be referred, by any fair interpretation, to the name (Yaton-Melek) immediately preceding, just as the same words at the end of the second line are to be referred to Ešmun‘azar. Otherwise, the words מלך צדנם would certainly have been put between ברעשתרת and בן צדק. It is of course not absolutely necessary that the king should give himself this title here; still, in view of the uniform usage of all the other inscriptions of this series, as well as of the other extant inscriptions of this same dynasty, the omission would be a most remarkable one. The name of the king—whether Ešmun‘azar I., Tabnīt, Bod-‘Astart, or Ešmun‘azar II.—is elsewhere *invariably* followed immediately¹ by מלך צדנם, whether the name of the father is given or not. The analogy of the other inscribed stones of this temple deserves to be given especial weight.

The letter ן in the first line is very much more difficult in Professor Porter’s restoration, it seems to me, than in my own. The ancient stone-cutters often made mistakes; frequently in the omission of letters, but oftener in the careless substitution of a character closely resembling the one intended. It would be easy to collect many examples of this kind, and no such blunder could be more easily accounted for than the accidental cutting of ן in place of ם. But the insertion of an *extra* letter, bearing no resemblance to those on either side of it, would be a very unusual thing.

The phrase בן צדק, “legitimate son” (*in the construct state*) is undoubtedly possible, but is it not also highly improbable? It has no parallel, so far as I know, unless in the obscure phrase

¹ Except that in the Tabnīt inscription the title “Priest of ‘Astart” is inserted at this point.

צמח צדק in the Narnaka inscription,¹ in a mutilated passage (line 11) whose restoration is extremely doubtful. **צדק** is hardly the word that we should expect to see used in this way (in the construct state, rather than **ל' צדק**); still, the possibility of the idiom cannot fairly be questioned. The chief objection, after all, to supposing its use here is the obvious one, that the author of the inscription is Bod-'Astart himself. It would be a very strange thing for a king, in an inscription of this nature, to style himself "the legitimate son" of such and such a father.

Why the phrase **מלך מלכ**, "king of kings," should cause any difficulty, I am at a loss to understand. The assumption that such titles were employed only when they were "appropriate" is utterly groundless. In all ages and parts of the world, kings and potentates have delighted in grandiloquent titles which corresponded to no reality whatever. This particular title had been very familiar to the Semitic world, and in all probability was not infrequently used as a mere ornament. It was applied, for example, to the Palmyrene prince Septimius Odainath, who is called in an inscription² **מלך מלכא**, "king of kings," though he was not even king in his own land. We are, of course, not called upon to explain why Bod-'Astart omitted the name of his father in the other inscriptions of this series; but it is not impossible that he wished to compensate partially for the omission by inserting the complimentary title "king of kings" in this one case.

In regard to the space at the right of the letters on the upper one of the two small fragments, it may well be that Professor Porter is right in his contention that the word **צדנ** originally formed the beginning of the second line. This conclusion, if true, would not materially affect my own reading, however, for it is very easy to suppose that the stone is broken off at the left hand as well as at the right. This, in fact, was my first impression on looking at the photograph. The whole might be restored as follows:

¹ Lidzbarski, *Handbuch*, p. 422; Cooke, *North Sem. Inscriptions*, p. 82 f.

² See Cooke, *North Sem. Inscriptions*, pp. 264, 290.

ברעשתרת מלך צד[נ]ם בן צדקיתן מלך מלכ[ם] מלך
 צדנם בן בן מלך אשמנעזר מלך צדנם [אש בן]
 [א]ית הבת ז בן לאלי לאשמן שר קדש

But every conjecture is precarious, in dealing with these most puzzling inscriptions. Professor Porter will doubtless see good reason for objecting to my interpretation as decidedly as I do to his. Lidzbarski closes his review of my *Semitic Epigraphical Notes*, in the *Theologische Literaturzeitung*, March 19, 1904, col. 166 ff., with the words: "Wer will nach diesem Fetzen sagen, dass das Ganze so, und nicht anders geheissen hat?" Not I, for one! But it does seem to me to be important, in the case of just these most difficult and fragmentary inscriptions, to hold as far as possible to what is known and obvious, rather than to make wide excursions into the region of conjecture; and for this reason I myself much prefer Professor Porter's interpretation to that of Lidzbarski. Inasmuch as this stone came from the temple of Ešmūn, and its inscription is for the most part identical with the others which have been found there, it is the safest method of procedure to depart as little as possible from the typical form which is already assured. It is better to assume (so long as nothing absolutely forbids the assumption) that this inscription is of about the same extent as the others, and therefore nearly complete as we have it, than to conjecture a reading which would oblige us to suppose that long passages are missing. It is more reasonable to start with the presumption that this stone, also, bore an inscription which celebrated the completion of this one building, and named Bod-ʿAštart only as the builder, and to try to work out a restoration of the text on this basis, than to imagine that other buildings and builders are also designated, and that the second line contains mention of the king of Malaga. In comparison with such flights of fancy as these, the simple supposition that a stone-cutter or copyist made the every-day mistake of forgetting to put the cross-bar on his מ, thus leaving it ם, is tame and uninteresting indeed.

But we may still hope for further light from the temple itself, where Macridy Bey is now continuing his most important excavations with such thoroughness and skill. More inscriptions are certain to be uncovered, and it may be that among them will be found something that will elucidate one or another of the mysteries which now seem so hopeless.

I take this opportunity to add a few further comments suggested by the article in the *Theologische Literaturzeitung*, just mentioned. Lidzbarski has not everywhere rightly understood my interpretation of the typical inscription of this series. (1) I do not regard מִשַׁל as "in apposition to בִּרְעֵשְׁתָּהּ," though I admit that my translation would easily give this impression. It seems to me more likely that it is the participial predicate of a clause whose (pronominal) subject is suppressed. It will not do to put much weight on the "expected" order of words in the Phoenician sentence, for we know very little indeed about the usages of that language.

(2) "Belonging to Sidon," in the sense in which I understand the phrase, could not possibly be אִשׁ לְצִדְנָם. I suppose the districts mentioned to be *parts of the city of Sidon* (not, "the property of the Sidonians"), and this idea might very naturally be expressed by מִצִּדֹן.

(3) As for the phrase וְצִדֹן שָׂר (or וְצִדֹן שָׂר), I need not say again that I have no confidence in my suggested translation. Still, my attempt, as a mere guess, has its merits. It is the only attempt thus far, so far as I know, which yields a comprehensible meaning without doing violence to known rules of grammar. I myself feel more and more convinced that שָׂר is the denominative verb, "walled." Might the whole clause possibly be translated: "[He it is] who built [buildings], and walled the city of Sidon"?

(4) Lidzbarski objects to my rendering of the last sentence of the inscription, that if the verb בָּן were thus repeated it would need a suffix. But the omission of the pronominal suffix under just such circumstances is a commonplace of Hebrew grammar. There is not the least difficulty in supposing it here.

(5) The statement that the last clause in line 16 of the Ešmun-azar inscription is "exactly parallel" (genau parallel) to the corresponding clause in line 17 is not accurate. In the one case, the simple object-suffix is used; in the other, the name of the divinity is repeated. I have tried to suggest a plausible reason for this difference, and still think the argument a legitimate one.